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three times in a set of cups, and found that the lighter gave more than ten per cent more wind at five miles per hour, while there was no difference between the light and heavy cups in higher winds.

4. In the more recent trials in England, an anemometer was placed, with its axis horizontal, on the arm of a whirler, and the whirler rotated once and then suddenly stopped: the anemometer cups (supposedly from their momentum) continued to revolve. This experiment was certainly most remarkable. It is a little difficult to see what other result was to be expected. If any thing was to be learned, it could only be by stopping the cups at exactly the moment the whirler was stopped. This certainly does not elucidate in any way whatsoever the supposed inertia effect in an intermittent wind.

5. The crucial test in the English trials was made when the whirler was given an intermittent motion or one simulating a natural wind. The anemometer was placed on the end of the arm, and the velocity of the whirler was changed quite rapidly, ranging back and forth between forty and ten miles per hour,—a far greater fluctuation than can occur in the free air. Here, then, above all things else, we ought to get an inertia effect; but it was found that there was no difference in the record of the anemometer between the uniform and intermittent motion. The evidence seems to be overwhelming that the supposed momentum or inertia effect is purely imaginary.

We cannot sympathize with the feeling aroused in England by these experiments; namely, that the Robinson anemometer is untrustworthy. Undoubtedly the Kew instrument, with its 12-inch cups and 24-inch arms, is exceedingly clumsy, and should be discarded as soon as possible; but the experiments in this country have shown that with 4-inch cups and 6.72-inch arms the results are all that could be asked near ten miles per hour, and during about eighty per cent of our winds. It has also been demonstrated that an anemometer can be constructed which will give very good results over a large range of wind velocity. It is very certain that the Robinson anemometer is far ahead of any instrument that requires a vane. In the English trials a vane anemometer or air-meter gave much more

uniform results than any other instrument in the open air, but this was simply because the effect of the natural wind would be almost exactly counterbalanced on opposite sides of the whirl. In portions of the whirl where the wind would tend to accelerate the motion, the much more rapid whirler motion would tend to keep the vane normal to the arm; and, even if the vane had any influence, it would tend to turn the anemometer at an angle with the arm in such a way as to make it lose the proper speed which it would have in its normal position.

It seems probable that the anemometer problem has nearly reached its solution. What are now needed most of all are experiments with an anemometer, as light as practicable, and which will present a slightly greater proportional resistance to the higher winds than to the lighter, and possibly cause fewer whirls around the cups.

H. A. HAZEN.

Washington, April 11.

Supposed Aboriginal Fish-Weirs in Naaman's Creek, near Claymont, Del.

THE copy of my letter published by the Rev. Mr. Peet in Vol. XII. of the *American Antiquarian*, March, 1890, No. 2, is correct in some respects, and incorrect in others. So far, Mr. Peet has failed to prove that I ever used the word "pile-dwellings," or "river-dwellings." My denial is quite as good as his assertion in this respect, until the original letter be produced, and proven to be mine. I have already given a true version of this letter in a former communication to *Science*, and sufficient reasons for the use of the terms "pile-structures," "pile-ends" or "log-ends," and "stations." They are sufficiently clear explanations for any one to understand without danger of an erroneous impression. No repetition of this explanation is therefore necessary. I call Mr. Peet's particular attention to this assertion.

In the reproduction of my letter, published on p. 118, *American Antiquarian*, March, 1890, I desire to correct the following error: "The numerous suggestions that the pile-structures were fish-weirs is untenable," should read "the numerous suggestions that the pile-structures were fish-weirs is tenable;" and if my



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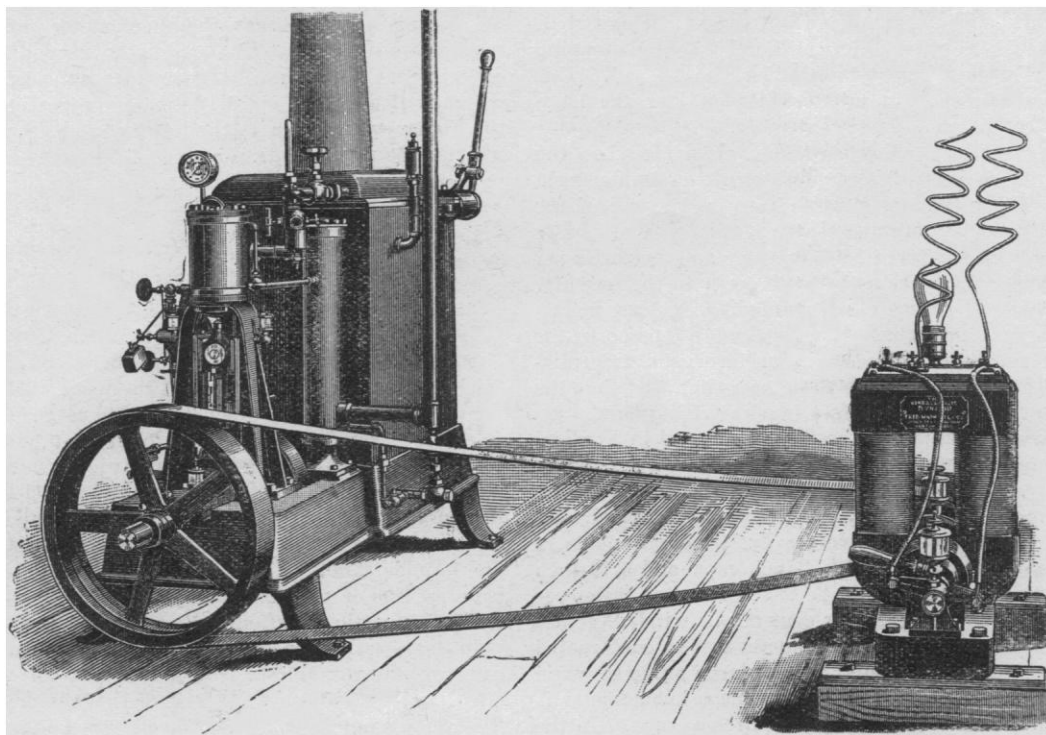
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letter in Mr. Peet's possession gives any other sense, it has been tampered with by some one desiring to make mischief. The remarks that I make at the end of this letter (written several years ago, and now for the first time published by Mr. Peet) show that I was opposed to theorizing upon the subject, and used the term "pile-structures" throughout the letter. Reasons for use of these terms, let me repeat have been referred to, as above stated. In regard to the quotations from the Peabody Museum

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A COMPACT ELECTRIC-LIGHTING PLANT.

reports, I have to say that its editor was perfectly free to express his opinions upon the subject, whether he accepted my fish-weir theory or not. So far, I have not yet made any definite report upon the subject, from lack of time to work the material up. There is no manuscript in the hands of the Peabody Museum, upon pile-dwellings or river-dwellings in North America, awaiting publication, as has been asserted. HILBORNE T. CRESSON.

Philadelphia, April 11.

oil-burning engine, which was illustrated and described in these columns about a year ago. The dynamo furnishes current for fifteen 32-candle-power incandescent lamps, six 25-candle-power lamps, and five 16-candle-power lamps. The fuel being oil, supplied automatically as required, and the water-supply, steam-pressure, and speed being automatically regulated, the plant requires scarcely any attention other than to start and stop it, and is said to give entire satisfaction.

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CATARRH.

Catarrhal Deafness—Hay Fever.

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